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the Corona, but the evidence with respect to the structures which have been visible in other coronas is not sufficiently full to allow of similar generalisations.

The fact that the light derived from the bright structures of the Corona as observed during total eclipses is strongly polarised shows that the brightness of the structures is not chiefly due to incandescent gas. It seems probable that the erupted matter must be quickly precipitated by the cold of space, leaving clouds of dust, which disperses the light derived from the photosphere. The strong polarisation of the light from the bright coronal structures seems to show that the precipitation gives rise to particles which are small compared with the wave-length of light. It is possible (as has frequently been suggested) that showers of metallic rain may also be produced, but the greater part of the light of the structures is evidently due to the light dispersed by the small non-incandescent particles.

Note on the Length of the Pendulum observed by De l'Isle de la Croyère at Archangel in 1728. By Major J. Herschel, R.E., F.R.S.

The observations at Archangel were made by Louis de l'Isle, brother of the geographer, William, and were recounted and communicated to the Academy of St. Petersburg by another brother, Joseph Nicholas, the astronomer, some years later—*i.e.* in 1735.

Their position in the series of pendulum observations should have made them of great importance, but two circumstances conspired to make them valueless. Of these, one was the delay in making them known; the other is the subject of this Note. A few words about the first may perhaps not be out of place.

The only pendulum observations which had obtained any repute were the following:—

1. 1670–1674, by Picard; at Paris, Uranibourg, La Haye, Lyons, and Cette;
2. 1671–1672, by Richer; at Cayenne and Paris;
3. 1681–1682, by Varin and Des Hayes; at Goree and Guadaloupe;
4. 1686, by Goüye; at Louveau (a place once the capital of Siam, but unknown to modern maps);
5. 1697–1698, by Couplet; at Paris, Lisbon, and Parayba;
6. 1699–1700, by Des Hayes; at Cayenne, Grenada, St. Christopher, and St. Domingo;
7. 1704–1705, by Feuillée; at Porto Bello, and Martinique.

Mention is sometimes made of a few others, but there are no authentic accounts; nor, indeed, are these, with one exception, of any present value except as historical indications of a certain kind. There is no trace (that I have met with) of any observation later than Feuillée's, until we come to De l'Isle's in 1728.

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It is further noteworthy that in no case are the early observations described in detail, in a way to permit of any check upon the result. In general the observation consisted of lengthening or shortening the line by which a bullet or ball was suspended, until its swings were sensibly synchronous with those of a clock pendulum. The length is then recorded, and in most cases the length so determined is erroneous to an extent which can certainly not be attributable to error of observation in the ordinary sense. I attribute it only partly to large unsuspected variation of clock-rate during the diurnal period. Such variation is by no means incompatible with a fairly uniform daily rate; and that daily rates of a really startling uniformity *were usual* in those days is abundantly in evidence: see, for instance, Richer's observations at Cayenne, only sixteen years after the invention of pendulum clocks.

The account of De l'Isle's observations, given at considerable length, is therefore the first of its kind, and is especially interesting for that reason—at any rate to us; though, as I have said, the delay in publication no doubt made it sink into insignificance by the time it reached Paris, where in 1735 the elaborate obsertions of Mairan were no doubt occupying attention. And I think it probable that the interest which might have attached to it on account of the high latitude of the station was destroyed by the near expectation or actual possession shortly afterwards of those obtained by Maupertuis in 1737–8 at Pello and Paris.

But for these untoward conditions, I imagine the result obtained at Archangel would have received more notice. As it was, the French *savans* paid no attention to it; but it was considered by Krafft in his Essay on the Figure of the Earth, in 1736 (published 1741).*

I need hardly say it has been noticed by all modern writers. It was not, however, among the fifteen selected by Laplace, but it is included among the "second class" observations arranged by Airy, where its apparent error catches the eye as being much the largest. One of the objects of this Note is to rescue it from that invidious distinction.

To come to the point: the writer of the account has made a curious (but patent) blunder in deducing from the actual observation of *rate* the *linear* result which custom then and always has prescribed. I will quote his words:—

"Longitudinem penduli simplicis constat esse in ratione inuersa radicum quadratarum numeri oscillationum tempore æquali peractarum."

And he proceeds to apply this as follows. The length being

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* Since this paper was written I have become acquainted with a very little known review of the early history of pendulum inquiry, viz. that of De Brémond, printed (as a note to his translation of the *Philosophical Transactions of the Royal Society*, for 1733 and 1734) in 1740. I hope to say more about this somewhat remarkable review elsewhere; for the present it will suffice to say that De Brémond falls into De l'Isle's error quite naturally.

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440·5 lines, the pendulum made 28815 oscillations in 8 hours—*i.e.* in 28800 seconds. Therefore, the length must be reduced in the proportion of the *square root* of 28800 to the *square root* of 28815; whence a reduction of ·115 upon 440·5.

But the proportion should be as the *squares*, not as the *square roots*; and the reduction should therefore be four times as great.

Following out the calculation, and making the necessary arithmetical corrections, without introducing modern changes, I find: that the difference of length between Paris and Archangel, which the author determines as between the extremes ·163 and ·144 line, or about three-twentieths, should have been between ·507 and ·430, or nearly half a line.

But the result is better seen by noting that the seconds pendulum is determined as being between 441·007 and 440·930 lines, without reduction for atmospheric effects—say 440·97. Now, the allowance for air would be, with such a ball as was used on this occasion, about +·12, making the concluded length equal to 441·09.

It is natural now to inquire whether this result is in accordance with what we know of the Earth's surface. To test observations expressed in this way, I am in the habit of applying formulæ which assume

(1) That the seconds pendulum at the equator is 991 millimetres in length, with a symbolical correction.

(2) That the compression is $\frac{1}{290}$, with a symbolical correction.

(3) That "Clairaut's fraction"* is therefore ·00522, with a symbolical correction.

The length of the seconds pendulum in latitude λ will therefore be, in millimetres,

$$991(1+\cdot00522\sin^2\lambda)+\text{ symbolical terms.}$$

To find the length in inches or lines we must substitute 39·0161 or 439·306 for 991.

Now, the latitude of Archangel is 64° 32′; and therefore $\sin^2\lambda = \cdot8151$; whence we may find the length of the seconds pendulum in the latitude of Archangel to be—

$$439\cdot306+2\cdot2932\times\cdot8151=441\cdot175\text{ nearly.}$$

De l'Isle would seem, therefore, to have been only ·09 of a line in error, supposing my assumptions justifiable, and that there is no local disturbance at Archangel to account for it. A disturbance of that magnitude would be large, but not incredible.

I have already written at greater length on this not very

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* This is the name given by Mr. Todhunter to the ratio of the increment of gravity from equator to pole to the force of gravity at the equator. The word 'therefore' in the text refers to Clairaut's Theorem.